E-2

POKREFKE

2^{ND} DRAFT COMMENTS (W/ WRITTEN COMMENTS BY GAINES)

Comments on Micro-Model Comparison Studies
Draft Number 2

1. As requested, I have reviewed the subject draft. I probably did not review it to the depth that I reviewed Andy Gaines dissertation, but much of this document came from his dissertation. I gave my comments on the dissertation directly to Andy this past summer in St. Louis, so any comments that I made on that document, which were not addressed by Andy, would still be applicable. I have some *general/typo-type* comments and then some specific comments on the Draft Number 2.

2. I have read the comments that Charles Nickles has provided to Andy, and I concur with what he has provided. In an effort to be as brief as possible and to make addressing all of the comments easier for the researchers, I will not repeat concerns/issues raised by Charles.

General/Typo-Comments

3. Comments are:
   a) **Page 1-4, Line 3**. Remove the first "and." done
   b) **Page 2-1, Line 1**. The word "arbitrary" was used. I believe LWRP is anything but arbitrary and is inappropriate to use, at least in the case of WES models. — LWRP represents the 97% duration flowline (WSP). The decision to use the 97% dur. WSP was made many years ago, but the decision was arbitrary because other WSP could have been used with equal effect. For instance, recent studies o[f] the White R. used a 95% duration WSP to define a LWRP.
   c) **Page 2-16, presented equation**. Up until this point "H" was used for depth, but starting here "D" was used. Need some consistency throughout the report and study. change D to H done
   d) **Page 2-16, first three lines under Section 2.2.2**. Line 1 – remove "of individual," Line 2 – remove one of the "of" after the word "calculation," and Line 3 – believe "an" should be "and." done

Specific Comments

4. Comments are:
   a) **Page 1-7, last paragraph**. States that the various parameters "…offer insight into the two-dimensional character of the flow." Since we are dealing with a riverine system and open channel flow, as a researcher I want insight into the three-dimensional character of the flow. If all I can get is two-dimensions, I can't really address the problems I am trying to solve.
   b) **Page 2-14, Section 2.1.5.2**. Text spent a far amount of time talking about weighted values, but I saw no evidence where "volume" was ever used in the analysis. Used in calculation of Reach Wtd values shown in T3-6 & 3-7 Weighting Not used Scratch Sect 2.1.5.2 in its entirety
   c) **Page 2-17, Section 2.2.2**. I personally have a problem with making an issue of the prototype variability. No doubt it exists and no doubt that most river engineers and riverine modelers know that it exists, but it is virtually impossible to qualify, forget about quantifying. The number of prototype surveys available to any researcher limits them, and they may still miss much of the "story" for any particular reach. The reason for including this discussion is that some (river engineers & modelers included) do not realize that prototype variability exists nor do they realize how to "deal" with prot. var. during the modeling process.

I have no definite answer why the data for the WES model are considerably lower than the MM. However, the WES model covered a shorter reach than did the MM.

d) **Page 2-21, Table 2-4**. I had trouble with this table in Andy's dissertation and continue to have it here. Completely ignoring any model data for the moment, what this Table tells me is that for the 1975 prototype conditions, the area (I'll only focus on one parameter) from a 1:8,000 or 1:16,000 scale model map is about 27% greater than from a 1:300 scale model map. This continues to dumb-fond me that there is such a huge difference when all we are really looking at here is **prototype data**. Part of it may be averaging 28 ranges on the 1:300 scale verses more than 70 ranges on the 1:8,000 and 1:16,000 scales, but that just seems like an unreasonable discrepancy that needs to be addressed.

e) **Page 2-22, Section 2.2.3**. You appear to spend this whole section making an argument for using +20-ft LWRP as a reference for the computations, and then in the end use 0-ft LWRP anyway.

intended emphasis is on ~~The section~~ The need to consider several elevations in diff calculations.

f) **Page 2-26, Table 2-4**. Are the values here percentages or what? Average Differences and Mean Squared Error

see pg 2-22 & 2-23 #1 & 2 last ¶

g) **Page 2-25, Figure 2-7**. Just a comment about this figure. I hope someone other than Andy understands it, because I sure don't. Figure demonstrates that differences between model & prototype Areas are greater when using Lower

h) **Pages 3-20 through 3-24 and all similar figures to these throughout the report**. These figures are the most enlightening to me and helpful in evaluating model performance. Noted.

i) **Page 3-26, Section 3.1.4**. Although I have heard Andy say this throughout my involvement on this evaluation effort, this really was not "predictive" in that the micro-model was operated with structures constructed in 1999 and results compared to a 1998 prototype survey. There appears to be a disconnect here. Even if the micro-model results would be compared to the 2001 prototype survey (which is included in Table 3-6 on page 3-37) this looks like nothing more than a recalibration or check of the initial calibration.

~~dike~~ construction in 1999 for the model reach consisted only of 300 L.f. minor repairs on dike 14 KA. Therefore model represented dikes constructed through 1999. Similarly, construction in 1998 consisted only of 1000 L.f. minor repairs to K.A. Dike 3T. ~~Earlier work~~ Prior to 1998, the last repair work was in 1993. The last major dike const. was in 1983. The last revetment construction within the model reach was in 1997.

be predictive, or plan, run was to compare the model's ability to assess prototype response a given change. In other words, could a calibrated model "predict" what would occur in the prototype. In essence, this is like a check on model calibration.

j) **Page 3-36, Section 3.2**. In my opinion, repeatability in any type model can be approached in two ways – (1) by initiating testing from the same starting conditions and seeing if the same ending conditions over the same number of hydrographs occur (with some "natural scatter"), or (2) by starting "the clock" after the model has reached **stability** (based on the researchers definition) and running a fixed, but meaningful (whatever that is) number of hydrographs and determine how "stable" the model was over the entire testing time. The results presented from the Kate Aubrey 1:16,000-scale micro-model in Section 3.2.1 follow the 2nd approach. In my opinion, the resulting overall variance presented on page 3-44 does not provide a high degree of confidence. The Jefferson Barracks micro-model study presented in Section 3.2.2 also appeared to follow the 2nd approach; however, the tests were conducted with a constant discharge, which significantly (in my opinion) reduces the variables in the testing. Regardless of this fact, the variance presented on page 3-49 is quite **impressive**. The most meaningful comparison (at least relative to large-scale WES-type models and micro-models) is presented in Section 3.2.3. It was stated that Rob had addressed multiple base test runs in his thesis, but I was unable to find that discussion. However, my point here would be that in the results of the Dogtooth Bend Reach presented on page 3-50 of the report, it is entirely possible that the comparison of the WES model is from the 1st approach above and the micro-model is from the 2nd approach. Such a comparison is not valid because the WES model was working toward "stability," and the micro-model had reached "stability."

Rob's Thesis states that "depths were collected at the same relative cross section locations from 7 individual base test runs of the DB model at WES." My understanding of the WES base test was that the model was ~~a point~~ operated to stability to form the BT bathymetry. This would be in the 2nd approach described in this comment.

5. I looked through the remainder of the subject report, but did not do an in-depth analysis of the results. I made a sincere effort to try and understand what was being said about "truncation" in Section 2.2.4 and Appendix A, but I was unable to see the value in it. Figures like A-5 through A-10 just did not provide me with any meaningful information. Noted.

6. I applaud Andy for Appendices B and C. There is some good data and information there that shed a lot of light on both WES models and micro-models, and it is great to see it in one location and not lost forever. To that end, I came to the realization that the method classically used at WES and with micro-models by comparing prototype and model surveys to determine the verification of WES models and calibration/base test of micro-models is probably the best procedure. Quantification of parameters can be done, but when all is said and done, I believe leaving it to the modeler to determine the fact as to the verification or calibration of the model or micro-model, respectively, puts the control and reasoning in the correct place. However, I think that the quantifications, as presented, would be of value to the modeler in evaluating limitations and departures from prototype conditions that could be helpful in evaluating and reporting predictive tests.

Noted.

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19 Sep 02

Comments "Comparison Studies – Draft No. 2"

1. Paragraph numbers sure would make commenting easier. Since there are no numbers I will describe the location by page number and paragraph on that page. noted.

2. *Page 1-6, 1st Par, last sentence*; It may be true that discharge data is not available for all of the models, but you should state the whole truth. If you had discharge data for every minute you still could not use the data since Micro-Model (MM) technology ignores it. The prototype data and WES model data in this report has associated stage and discharge hydrographs and I expect that most of the MM study reaches have both water surface and discharge data that was not obtained because it is not used. I know for fact that for the studies of the Kate Aubrey reach, there is plenty of discharge data available. Since the MM technology cannot scale nor reproduce stages nor discharges, it is wrong to blame "lack of data" as the excuse for ignoring the single most contributor (stage/discharge) to a river bed form. It is the shortcoming of the MM technology not lack of data….and should be reported that way. Bankfull discharges, and discharges at +20 and 0 LWRP were not available from data provided on WES models or the prototype. The lack of discharge data, whatever the reason, means that the geomorphic variable of Q could not be analyzed for this study.

3. *Page 1-6, 3rd Par, all*; The Webster's dictionary defines thalweg as

   "**1a:** a line following the lowest part of a valley whether under water or not **b:** the line of continuous maximum descent from any point on a land surface or one crossing all contour lines at right angles **c:** subsurface water percolating beneath and in the same direction as a surface stream course **2:** the middle of the chief navigable channel of a waterway which constitutes a boundary line between states"

   This definition allows a lot of latitude to the modeler to define the location of the thalweg. From page 1-4 you state "the real comparisons of whether a model was considered calibrated/verified depended on the visual interpretation of the model or prototype bathymetry by the respective modeler(s) as opposed to any rigorous technique. The current research identified a need for quantitative comparison data to facilitate evaluation of the morphologic similarity criteria." It seems to me that you are substituting one interpretive method with an other. I more closely followed part **2:** of the above definition whereas someone else could connect deep point to deep point for each cross-section. Both could be considered right. I think you should make it clear that the thalweg location is **qualitative** not quantitative. Comment noted and attempt will be made to clarify.

4. *Page 1-8, 2nd Par, last line*; I do not agree with this statement. For a historic calibration like used for the WES models, a large degree of change between the beginning and ending surveys is good. This indicates the reach is active and is dominated by the hydrograph (stage/discharge), which gives the modeler a clue to the parameters that need to be adjusted. Also surveys taken during high water or low water could differ greatly. I can see where a high degree of variability could make MM calibration difficult. I could not have any confidence in a MM

calibrated to highly variable surveys, because the regime of the model and prototype are so different. A high degree of variability indicates to me a unstable reach which means the sediment load entering and leaving the reach are not stable whereas the MM results are (supposedly). This statement links prod. var. to the difficulty in modeling a particular reach. Likewise, calibration of any model is complic. by higher prod. var. Does your experience tell you that it is easier to achieve verification when the "parameters" require more adjustment than when they don't.

5. *Page 2-1, 2.1 Methodology, all*; How did you account for the slope of the LWRP when using the MM data. For example in the WES Kate Aubrey model there was 8 total feet of slope in the LWRP over the reached modeled and it was not evenly distributed. The prototype surveys and the WES models adjusted the elevation referenced to LWRP as the LWRP changed. How was this done for MM data? LWRP was not adjusted Based on my knowledge the MM is surveyed as a distance below the top of the insert and the elevation is of "0" is set by the shift. Which means that 0 LWRP is a fixed distance below the top of the insert and the only slope in it is the table slope. You can in no way assume the table slope is equal to the LWRP slope, because the prototype LWRP slope is relative to the channel centerline, whereas the slope in the MM channel is relative to the direction the channel is running on the table. For example in the New Madrid MM, the table had positive slope but because of the channel orientation on the table most of the channel along the centerline had no slope and in places a negative slope. Also supplementary slope is sometimes required to make the model sediment move and must be removed from the survey for comparison to the prototype. If you consider the Kate Aubrey reach model in the WES model, if the parameters you calculate that are based on the cross-section shape below an el. referred to the LWRP are not adjusted for the LWRP slope (to match the prototype), then the parameter are invalid to compare to the prototype or WES models whose calculated parameters are adjusted for the LWRP slope. If the MM cross-sections were not adjusted to reflect the LWRP slope, then any calculated parameter derived from an area or length of section below a specific LWRP elevation compared to the same parameters from the prototype or WES model cross-section are **TOTALLY INVALID**. NO! And should be deleted from this report.

OVER →

6. *Page2-22,Par3,sent. 1*; All the models in this report except for the MMs <u>did reproduce</u> bankfull stages and higher, give me some examples of models other than MMs that use a hydrograph type input that did not reproduce bankfull stages. Also the you stated that the MM typically reproduce stages up to +20 LWRP, I question this since stage is not controlled nor measured in a MM, show me some data to prove me wrong. If you cannot prove it with valid data then do not state it!! Comment deleted.

Here's your data — mm dike elev. are set to appropriate LWRP elev (~typ +15) and model flow at max Q overtops these elevations. Point checks with the digitizer also serve as confirmation of max stage of ± +20 ± LWRP

7. *Page 2-22 & 23, Last Par*; I am totally disgusted with this whole discussion, I agree the selection of the water surface will change the numbers, **<u>but the fact remains the data does not change.</u>** All you are doing is massaging the data to say what you want. Pick an elevation, use it throughout and stop insulting the reader with this silliness. This is like changing the vertical scale on a graphical plot to make a line straight or wavy, but it does not change the facts!! You used 7 pages of text, one figure and one table to discuss something that is unimportant

and does not change the results of this report, all it does is change the magnitude of some numbers. Also if the water surface elevation you select for calculating any of the parameters intersects one or both of the vertical sides of the MM insert, it should not be used. The cross-section is totally dissimilar to the prototype because the bank line was formed by a saw cut that on a line drawn by the modeler on a photo that is independent of elevation and is cannot be considered accurately similar to the prototype either location or elevation. No Reply Warrant

8. *Page2-23, Last Par*: I think the table reference should be Table 2-4 instead of 2-5. corrected 2-4 to 2-5 – Table incorrectly labeled.

9. *Page2-24,Par 1, Sent. 1*; You stated "there is more agreement between model and prototype surveys using an elevation of +20 LWRP than when using elevation 0 LWRP." How in the world do you get **more agreement** if you are using the same two cross-sections, impossible? The agreement **did not change**, **you** changed a number. I again reiterate my comment no. 7. If you have 2 apples and cut them in half, you still have 2 apples. True you have more pieces but still only 2 apples. Should state smaller differences result. Will clarify.

10. *Page 2-27, Part 2.2.4.*; I read this and Appendix A, and have one question "What?" If it took a paragraph and a 15 page Appendix with numerous data calculations to describe a "simplistic approach", I am glad it was not a complicated approach! Really, it seems to me that if you compare identical section of the model and prototype cross-sections you would be okay and could eliminate Appendix A. (Simplistic approach is depicted by Fig A-1. The remainder just illustrates the effect that result from truncation

11. *Page3-2, Par 2, Sent. 3*; I thought the ranges were the same for prototype and all models. I know the ranges in the WES model coincide with the prototype survey ranges of 1975 and 1976.

12. *Page 3-2, Sect. 3.1.2*; See Sect 3.1.2-redo.doc for suggested changes. noted. Changes adopted with modification as shown.

13. *Page 3-11, Table 3-1, Thalweg Position*; Here again I say a calculated thalweg position is a poor parameter for comparison since its location is subjective to the drawer. I used my own criteria to draw the thalweg position for the Prototype and WES models, but the location was likely not drawn on the MMs using the same criteria. For example, if you look at the WES model verification results, I could have just as easily drawn the crossing further upstream between ranges 20 and 25, thus the model thalweg would have plotted on top of the prototype plots. Also because the WES model bank lines have to be laid back (top of the bank moved away from the channel and the toe moved toward channel) in order to be stable and the MM bank lines are vertical some where near the location of the top of bank, a point to measure from on the either model data that would exactly coincide with a point in the prototype is impossible. Visual assessment and evaluation is the only fair way to compare model and prototype location. Opinion noted – emphasizes that care should be used in defining thalweg location.

be sure to include entire quote

14. *Page 3-17, Par 1, 1st Sentence*; You state "Equilibrium in the small-scale models represented the condition where sediment transport and the bed bathymetry remained consistent for successive cycles". (there was no net agg/deg over time observed in the model). MM technology has no method to know if sediment transport is in equilibrium. During my work on the Kate Aubrey model at your lab, Wayne and I did test where we run the model numerous cycles and surveyed after every 3 to 5 cycles. The model did not ever reach equilibrium as described above. It did seem to cycle bed forms every 10 to 15 cycles. This data were recorded and should be in your system unless it has been destroyed. These results show the sediment transport through the model never reaches equilibrium; this makes the modelers decision as to "stability" very critical and should be very careful to be consistent. see remainder of sentence quoted

15. *Page 3-25, Par 1, Sent. 1*; The 1973 survey should never have been considered The 1973 bed form was produced by a hydrograph that cannot in no way be replicated in the MM for either stage or discharge. The 1973 Prototype is in no way a typical (average) bed you can expect in the reach. The one thing this comparison does indicate to me that I would not want to use the model as calibrated to test any alternatives. This data (along with cross-section plots I made of the 1:16,000 scale model and the three prototype surveys) indicate the MM does a fairly good job of reproducing the 1973 bed form, but not the 1975 nor 1976 bed forms. (what plots - were these of the final calibrated MM - 022301d surveys? I would not want to use a model to evaluate alternatives that most closely reproduces the bed forms caused by the third largest flood of record on the Lower Mississippi River. If you really think about it, the large distortion, steep slope and extremely high velocities that are inherent to the MM would most closely reproduced high flood conditions. Comment Noted. current Data do not support the conclusion that MM reproduced 73 channel

16. *Page 3-36, last Par, 1st Sent*; I think you left out a critical boundary condition, initial bed configuration. Noted. Earlier in this report you discuss model and sediment transport be in equilibrium, in this paragraph you tell me sediment transport by nature cannot be in equilibrium. Am I to deduce that the model cannot be in equilibrium either? I cannot read you mind, tell me what you believe and stick to it. Changing sides of the fence in wishy-washy. 1st # p3-17 states "Eq. in S-S models ... there was no net aggradation/deg. over time." This does not imply no variation in local values sediment motion

10 deepest but same as 20
20 deepest
30 deepest - same as
40 middle
50 ~same as 20
60 lowest
70 highest

17. *Page 3-39, Sect. 3.2.1*; This paragraph needs some work. The way it reads is confusing and difficult to get your meaning. How many cycles were reproduced to develop 022301d? NA It is the What is the starting bed form used to initiate series of cycle representations? The thing that jumps out at me when I look at the cross-section plots is for the 022801a (last survey) that in the bend type sections the thalweg is generally the deepest and in the crossing type sections the thalweg is generally the shallowest. This indicated to me instability, not variability. disagree I do not remember the exact time frame that I did the sensitivity test, but there should be a large number of surveys associated with, because we ran sowething like 160 cycles. The plots of that data show a definite cycle to the bed form and that it repeated. noted These surveys do not reflect any of the repeatability runs made Fall 1999 because of problems with model calibration. These surveys shown begin with a calibrated model bed and then continue simulations by # cycles shown.

18. *Page 3-49, Par 1, Sent 2*; Shame-Shame…In research, altering a data set without concrete proof and then using the altered data to influence your conclusion and

results is a **BIG NO-NO.** You cannot use the suspect data if you believe it in error but to alter it, you destoryed the integrity of the data set and any conclusions derived from it.

Spikes were removed — Dave respond on the JB variance analysis.

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3.1.2. Large-Scale Models. An example of large-scale model is the Kate-Aubrey model of the Mississippi River conducted by WES. A photograph of the large-scale Kate-Aubrey physical sediment model is shown in Figure 3-3. The Kate-Aubrey reach is located north of Memphis, Tennessee between river miles 785 and 797. The purpose of the study was to develop a ~~structural~~ plan ~~the~~ to reduce or eliminate ~~determine~~ the extent of shoaling between river miles 788 and 792.5. The model ~~used for the study~~ was a loose-bed model with crushed coal as the bed ~~sediment~~ material. ~~and~~ The model was constructed to scales of 1:300 horizontal and 1:100 vertical (model to prototype, respectively). The coal had a median diameter of 4 mm and a specific gravity of 1.30. Prototype data used in this study were bathymetric surveys for May 1975 and May 1976. Prototype bathymetry for 1975 and 1976 are shown in Figures 3-4 and 3-5, respectively. The model bed configuration and structures (e.g. dike fields) was initially formed (or molded) to the 1975 prototype bathymetry. A model ~~stage and~~ discharge hydrograph~~s were~~ was developed from historical stage and discharge records for the prototype from May 1975 to May 1976. The resulting hydrograph~~s~~ (also referred to as the verification hydrograph~~s~~) ~~were~~ was used to simulate the historical period in the model between ~~the two bathymetric surveys~~ ~~May 1975 and May 1976~~ ~~in the model~~. The model discharge was distorted by a factor called the discharge ratio, which is adjusted during the verification period to insure proper bed sediment movement and model bed response. Model s~~S~~ediment material was manually input and recorded at ~~added to~~ the upstream end of the model ~~(a sediment feed system was used)~~ during simulations to develop a model stage vs. ~~to maintain a desired rate of sediment load relative to water discharge for the reach. This produced a~~ model sediment input rating curve. The model slope, rate of sediment input, ~~load and water~~ discharge ratio, and boundary conditions (e.g. ~~tailgate setting and~~ bank roughness) were adjusted over the course of several repetitions until the final model bathymetry reasonably reproduced the May 1976 prototype conditions. (NOTE: The tailgate is used to maintain the proper water surface in the model, therefore in almost totally dependent on the model discharge. That is why I, and I underline I, do not consider it a separate boundary condition.) Each repetition began with the May 1975 prototype bathymetry ~~condition~~ formed in the model. The model was then subjected to ~~used to simulate~~ the verification hydrograph~~s~~ ~~(including the corresponding sediment rating curve)~~ to obtain a model bathymetry to compare with the May 1976 prototype survey. The large-scale models employed a verification process to establish the ~~basic~~ model operating parameters. The verification procedure relied on a visual comparison of model and prototype bathymetry as described in Gaines (2002) and was considered verified when the model bathymetry reasonably reproduced ~~. Once~~ the May 1976 prototype condition. ~~was reproduced in the model, the model was considered verified.~~ Model bathymetry after verification is shown in Figure 3-6.